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CHANCE

MY purpose is to show that chance is an objective category; objective, that is, in the same sense as causation, space, quantity, or other accepted scientific categories. By a chance-event, I mean an event which has no cause; though a fuller definition will appear in the course of the argument. The question of the ultimate metaphysical status of the category will not be discussed.

That there are aspects of the physical world which are, in a sense, outside the pale of law and causation, is widely admitted among philosophers to-day. Professor Royce has shown[1] that the element of significance or value which resides in individual things can not be scientifically accounted for; Mr. C. S. Peirce has argued[2] for an ultimate indeterminism out of which grows a certain amount of law; James[3] and Bergson[4] have defended an irreducible spontaneity in all real events; Professor Palmer has lately advocated chance-combinations of causal series;[5] Cournot[6] and others in France have stood for a similar view. Admitting in general the truth of these positions, I wish to carry the argument somewhat further, to give the concept a more positive interpretation, and to place it firmly within the field of scientific categories. Not only is chance, as I believe, more than a mere name for our ignorance; not only is there a certain aspect of fact which is outside of causality; there is a perfectly definable, intelligible tendency in physical events toward variation from law, and this tendency is nearly, if not quite, as widely verified as laws themselves. I shall venture, then, to differ from

[1] "Spirit of Modern Philosophy," Lecture XII.

[2] *Monist*, January, 1891, April, 1892; also, incidentally, in October, 1892, January, 1893, and July, 1893.

[3] "Some Problems in Philosophy," Chapter IV.

[4] Principally in "L'évolution créatrice." As this is one of the main contentions of the whole book, specific reference is perhaps not needed.

[5] In "The Problem of Freedom," Chapter X.

[6] "Essai sur les fondements de nos connaissances." Many articles on the subject by others have appeared in the *Revue Philosophique*.

most of the previous views in regarding chance as a well-defined and, in one sense, a positive category *within* the scientific field or world of description.

Before we go to the evidence for this view, a word must be said as regards the subject of this investigation. It is not the pure or mathematical concept of chance which is here studied, but the empirical; and a failure to distinguish these might lead to misconception or misdirected refutation. The philosophy of scientific categories, toward which this paper aims to contribute, may proceed in either of two ways. It may study such categories *in abstracto,* as pure concepts and members of an ideal system of concepts, without direct concern as to their mode of application to experience; or it may study them, not as members of an ideal system of knowledge, but as their nature is revealed in actual scientific treatment of the facts to which they apply. The former method treats categories as instruments of exact knowledge and perfect determination, a purposive rearrangement of data, due entirely to the activity of mind, and dominated by its ideal purposes; the latter treats them as adaptations, rather, in which the ideals of the mind are less dominant and the intelligence of the knower is more subjected to the data. Examples of the former are the many recent works upon exact logic; of the latter, Bergson's definition of consciousness in "Matière et Mémoire," Dewey's definition of truth in "Studies in Logical Theory," Montague's definition of consciousness in the paper "Consciousness a Form of Energy."[7] In general the results of these methods will not agree, because they study different concepts. Causation as a factor in an ideal system of knowledge may be a very different thing from the causation that is used in the science of to-day. But to the philosopher both should be at least interesting. As to the question, which one is the ultimately correct category, that lies beyond the province of this paper.[8] I consciously choose the empirical concept of chance, seeking to know what, if anything, of the fortuitous is implied in the scientific methods and results of our time.

If we consider the world in cross-section, at one moment we seem to find many causes acting, which themselves bear little if any causal relation to one another. That I am at this moment speaking can not be causally explained, so far as we know, by the fact that the tide is just now turning in the harbor. That the tile on the roof is loosened by the wind and falls just at the moment I pass beneath it (to use the familiar example) may very well be fully determined by ante-

[7] In "Essays Philosophical and Psychological in Honor of William James."

[8] The clearest statement I have found, of the ideal or conceptual method, is in Professor Royce's "William James and Other Essays," pages 234 ff.

cedent causes; that I pass at that moment may be equally determined; but each of these series seems to be quite undetermined by the other. Here appears a loophole through which chance might enter the scientific realm; many thinkers have been so persuaded. And yet who knows that further scientific evidence might not show the two events related as the scales of a balance? It might be a case of Kantian reciprocity. My passing beneath the house jars the earth, the house, and the tile, however slightly; and if the tide did not turn just now when I speak, something would be wrong with the moon or sun, and who knows what meteorological conditions might immediately transpire, even to the destruction of all of us? It seems to me that we must await evidence on this point. Meanwhile I find nothing in the observed results to rule out a mutual determination of all these facts.

But there is another way in which events might be uncaused. We might consider, not a cross-section of the world at one moment, but a sequence. It is conceivable, whether credible or not, that in a series like the successive positions of a falling body slight variations from the straight path might occur, which were not caused by anything in the past history of that body or any other fact, past or present. Is there any evidence, in present scientific methods or results, of such phenomena? Is there any direct and positive implication of *uncaused* variations from exact law in any of the sequences of this world?

At the present date we have, thanks to the accurate measurements and tabulations of anthropologists, biologists, economists, meteorologists, and others who employ statistical methods, an enormous body of facts of the sort we are seeking. It has been found, for example, that the height of men and women, the length of various organs, the fluctuations of the thermometer, of rainfall, of prices, and so on, show a variation about a more or less ideal type or average. And what is more, the manner of variation is much the same throughout. To quote Professor Pearson: "From paupers to cricket scores, from school-board classes to ox-eye daisies, from crustacea to birth-rates, we find almost universally the same laws of frequency."[9] Nor is such variation confined to phenomena of living organisms. Besides meteorological facts already mentioned, we find that the exactest measurements in our physical laboratories show similar variations in the facts there recorded.[10] But with the exact delimitation of the field wherein such variations occur we are not concerned; enough that they are widely prevalent.

[9] "Chances of Death and Other Essays," page 20.

[10] Any work on statistics will give an idea of the wide extent of this fact of variation. See, *e. g.*, G. U. Yule, "Introduction to the Theory of Statistics."

We may say summarily that there seems to be a tendency, when experiments are repeated again and again, for the results to vary more or less about an ideal standard, norm, or type. For we may regard each human individual, say, as a repetition of the experiment of producing a human being; each rainstorm as nature's repeated attempt to produce rain, etc. That many such experiments are being conducted simultaneously does not affect the logic of the situation, just as the result is indifferent whether we toss one penny many times or many pennies at once. The examination of large collections, or repetitions of similar phenomena, thus suggests what we could not discover from the single case, namely, that besides the general law which says "be so and so" there is another which says "be not quite so and so." Such at least is the superficial impression we get from the facts. Indeed it seems likely that had the science of statistics been organized as long ago as the other natural sciences, philosophers would scarcely have defended universal causation as frequently as they have done.

But superficial impression is far from demonstration. The mere fact of a wide-spread tendency to vary from a type is hardly the slightest evidence of real chance. Are not all the variations themselves caused? If you are taller than I am, surely there is a reason for it; if to-day's rain is heavier than last week's, atmospheric conditions will account for it. But let us look again at the variations. We said above that their manner was much the same everywhere. And, moreover, that manner is a rather remarkable one. When the numerical values are graphically plotted they reveal a fairly close approximation to the well-known curve of error, or probability-curve. *Exact* correspondence with that curve we do not get, of course; but perhaps no concept, curve, or standard ever fitted the facts exactly. Laws are certainly never exactly fulfilled, yet we accept them. Now this striking unanimity of the variations suggests that they are not completely accounted for, each by its particular causal antecedents, but that a special tendency must be invoked to account for this common property; and that, too, a tendency to vary fortuitously, since the probability-curve is just what would result, in the long run, from fortuitous variation. If a series of murders are committed in a city, or in several cities, with a cross drawn in blood on the forehead of every victim, we should reasonably infer that one man, or band of men, was the author of the crimes. Such coincidence would be the strongest kind of circumstantial evidence. Our case seems just like that. It is hard to resist the conclusion that there is a wide-spread tendency at work in nature, making each event a little different from what it would be if all were governed by absolute law.

But the belief in universal causation, at least within the sphere of science, is so ingrained in us by our modern education that it is not easily dislodged. *Must* this resemblance to the probability-curve be explained by a fortuitous tendency to vary? For, if there is anything less than a strong logical compulsion here, we can hardly abandon that widely attested concept of law. We must then ask whether the facts could not possibly be explained without the resort to chance. And in answer I shall try to show, first, that a special tendency to vary must be begged, and secondly, that this tendency must be such as to permit chance to the individual cases, though not to the group as a whole. No other explanation of the situation, I shall claim, will do justice to the facts.

First, then, can not the resemblance to the probability-curve be explained on the hypothesis of universal causation? Let us see how that hypothesis would work out. Consider the case of the heights of a large number of men in a given city. When their numerical values are plotted, we have an approximation to the said curve. The height of each man is undoubtedly dependent on many causes, such as inheritance, nourishment during years of growth, early health, open air, sunlight, amount of fatigue in early life, etc. Now if you take a great number of men, these causes are certain to vary greatly from man to man. They will combine very differently in the individual men, giving very different results. And if you take men enough, you will include all possible combinations of these many causes. And this is no affair of chance, but is certain to be the case. Every possible effect upon the height of a man will thus be realized, and this, as is well known, will give a result approximating the curve. No special tendency toward variation need be conjured up, therefore; the large number of ways in which the causes affecting growth will combine, guaranteed by the large number of men measured, will suffice to account for the facts. So much for the hypothesis of causation. As a matter of fact, it seems to be the view of many writers on the subject.[11] Yet I can not but regard it as unsatisfactory. That each variation is indeed due to many cooperating causes is indisputable. That it can be *wholly* explained by those causes is a very different matter. For it is a condition of the formation of the curve that all possible combinations be realized in equal numbers. And there is nothing in the causal explanation to ensure this. The mere fact that by taking cases over a wide enough area you get all possible combinations of causes will not determine that those different combinations occur in anything like approximately equal

[11] *E. g.*, Venn, "Logic of Chance," page 475, footnote. Jevons, "Principles of Science," page 196. Laplace, "Philosophical Essay on Probabilities," page 4.

numbers. But they do so occur. Accordingly, I think we are driven to say that over and above the known and unknown causal laws there is a special tendency, active in nature, to realize in the long run every possible combination of causes in equal numbers. And since the individuals that vary, whether they be human heights, or organs, or prices, or temperatures, or what not, are themselves the products of many causes, we may perfectly well say that individual phenomena themselves tend to vary equally in all possible directions about a type. I say "in all *possible* directions," for the variation is always, apparently, restricted to a rather narrow field. But within that field, at any rate, a clearly marked and positive tendency, in addition to the usual kinds of causation, seems a necessary hypothesis.

Of course "tendency" is a vague word and renders one liable to the accusation of hypostasising an abstraction. But it is here used as no more than a concept or formula to summarize a large class of facts. Exactly the same is true of such concepts as causation and of the particular causal laws to which we accord our belief. In a sense they explain nothing and solve no mysteries. I do not here claim for the tendency in question any deeper validity than we ascribe to the usual causal laws; but if the argument so far is correct, it should have at least as much validity as those concepts have. We should speak of a real tendency among events to vary about a type, even as we speak of a real tendency in bodies to fall, or a real tendency in heat to radiate.

We come now to the second point mentioned above. May not this tendency to vary be itself a unique kind of a causal law, strictly determined in every detail? If it is so orderly and regular on the whole, must it not be equally so in every particular case? The probability-curve is a very regular affair, and the variations of phenomena are, on the whole, very regular too. We find approximately the same proportion of heights above the mode, the same below, again and again. How could the collection be so orderly if the individual members were lawless? In short, we must now examine the individual instances, to see how this collective tendency should be interpreted in its application to them.

If the tendency to vary is operative through the series as a whole, it can not well be *nil* in any one event. What form, then, must it assume in one such event? There must be a tendency for each event to vary somehow from the norm. And further, it must be either predominant in one direction, or equal in all directions. On the latter alternative, the various directions counterbalance one another, and nothing can decide which variation will occur except some cause external to the event itself, or just chance. But it will not suffice us to appeal to an external cause to decide the matter. For, as we have

seen above, the appeal to such causes will not account for the collective character of the variations. It will not guarantee what must be guaranteed, that the variations will, in the long run, be fairly equal in all possible directions. If some particular, external cause decided, in each instance, which of the conflicting directions should prevail, we should not, in general, have in the series as a whole the all-inclusive manner of varying that we do find. The only alternative is chance. This and this only would seem to allow to the series that elasticity which enables each instance so to combine with the others as to give the total result we observe. If then the tendency to vary is in each instance equal in all directions, the actual result in that instance must be ascribed to chance.

But perhaps in each case the tendency to deviate is strongest in one direction, changing in accordance with some fixed and unknown law as the cases are repeated, and gradually covering all possible cases. This again would seem to reduce all to strict causation. To be sure the variations seem to be essentially irregular and disorderly, but that may perhaps be due to our ignorance. May not the tendency to vary be itself found an orderly and thoroughly determined affair if we could only study it carefully enough? To this question I must answer, no. The collective tendency toward *variation* seems to me inconsistent with causal determination of the individual case. It is, I think, generally agreed within the scientific field that one and the same cause can not, under constant conditions, produce varying effects. The cause we are discussing is the *tendency to vary,* which is, perhaps, in *some* sense, one and the same throughout the series. In so far as it is the same it must be supposed to produce, under similar conditions, much the same results. Now the conditions in all the individual cases are, to all intents and purposes, the same throughout. For our tendency acts *independently* of these special circumstances of each case. We have already seen that those circumstances could not guarantee the nearly equal distribution which occurs, and that consequently the tendency in question must be begged; and its action must be the predominant one if the result is to be secured. Each variation might then be treated as if it were due to that tendency alone. But *that* seems to me equivalent to having the conditions constant: the tendency to vary acts as if it were in isolation. It produces, however, as the experiment is repeated, ever-differing results. As this would seem inconsistent with the causal action of the tendency, such action must be denied, and we must say that the individual variations could not possibly be caused by one tendency. Even if we discovered some time a hidden regularity about the variations, an order expressed by some function beyond our present knowledge, that order would have to be regarded

as fortuitous. For the fact that there occurred different results from one and the same cause would be, for science, an inexplicable thing. Is it answered: "Perhaps your tendency to vary is not one tendency but a manifold complex of them"? The same inconsistency with causation would, I believe, hold even then. In so far as the complexity obtains, it means, after all, *at bottom*, many independent (*i. e.*, fortuitous) tendencies. In short, no *one* tendency can explain an ever-varying manifold of effects, and *many* tendencies, in so far as they can not be reduced to one, themselves constitute chance. It is the spreading or multitude of the effects that, in my opinion, renders a causal explanation impossible.

In cases of ordinary causation, the same cause does indeed produce ever-varying effects. But that is because it acts in ever-differing circumstances, and its action is influenced by those circumstances. Our tendency however can not, in the long run, be influenced by them. It acts *with* them and *in* them, but it must predominate over them if the equal distribution is to result. And it is this predominance, or causal isolation in a certain sense, which is the key of the situation. The manifoldness of the effects has nothing left to explain it but just its own manifoldness. From one isolated principle you can never get many results, and the many results can not combine into just one isolated principle.

The conclusion thus seems to be forced upon us that our hypothesis of an all-inclusive collective variation implies complete ambiguity in the single case. We have then obtained, if the argument is correct, the following principle: there is a tendency, in many phenomena, to vary with equal frequency in all possible directions from obedience to law, the variation being such as to give regularity for the group as a whole, chance for the individual member. Of course this tendency is hardly ever, if ever, completely realized. It is a limiting concept, like that of law and causation. But it gives what is to my mind a more positive signification to chance than has usually been ascribed to that notion. Not *mere* irregularity, but a tendency to spread, to diverge, so as to treat all possibilities fairly and give them an equal showing—that, somewhat metaphorically expressed, is what I think we should mean by chance. Of course these possibilities are not absolutely infinite in any one case; they are always restricted by the special circumstances of that case. Men probably can not vary much in height; temperatures in a given region range hardly more than a few degrees out of the long scale known to science; and in general the field of chance is relatively small. On the other hand, we seem to find some amount of chance accompanying almost every case of law. How wide the field of variation is, in each class of phenomena, would seem to depend on the nature of the

causes whose combination gives rise to the phenomena. But the whole matter is an empirical one. Our view gives no occasion for those caricatures, as Professor James called them, which would accuse its advocates of believing that *anything* might happen in a given situation. Nor does it offer a contradiction to the principle of causality. Each variation is the resultant of many causes together with a chance-deviation. It would not be regarded as a denial of the law of gravitation if I held up a ball in my hand. No more does it deny the constant action of causes to assert that there is another principle cooperating with them. But the view I defend would imply partly uncaused beginnings, arising to some extent *ex nihilo*. Should a last stand be made on the ground that the principle of the conservation of energy would forbid any uncaused changes, we need only remember that the measurements which prove the conservation of energy are themselves subject to the same kind of variation as that we have been exhibiting.

Finally let me indicate the relation of the above view to some previous arguments for and against indeterminism. It is well known that the more we learn about any given event, and the finer our measurements become, so much the closer is the approximation to exact law. The conclusion seems to many thinkers to follow inevitably, that a perfect knowledge, measurement, etc., would reveal perfectly exact law. It seems to be a case of a variable approaching a limit, as a hyperbola approaches its asymptote, or the series $1 + \frac{1}{2} + \frac{1}{4} +$, etc., approaches the number 2. But the mere fact that we get gradually nearer and nearer to exact law does not imply that the latter is the limit we are approaching. If a line be drawn parallel to the asymptote and beyond it, the curve gets nearer and nearer to that line, but does not approach it as a limit; and the series $1 + \frac{1}{2} + \frac{1}{4} +$, etc., gets nearer and nearer to 3 without approaching it as a limit. Such reasoning is then quite inconclusive. Moreover, it overlooks the fact, which is the pivot of my argument, that the deviations from exact law themselves, when recorded and measured, show a positive manner of varying which can hardly be explained by causation. It is in this point that the present argument differs, so far as I know, from all previous arguments for indeterminism. Even those of Bergson and James, as I understand them, fail to point out this positive difference between law and variation. They find a fluent quality about facts which forever escapes the static and rigid concept. Yet one might reply to them that our concepts approach the fluent changing reality as a limit. Even though those concepts never reach that limit, they allow no irreducible remainder, which can be definitely named, to stay outside the conceptual series. The advocate of universal law may say: "You can point to no one fact

which I can not come nearer and nearer to accounting for completely." The series π is never completed, yet any one term of it, which you can name, may be exactly computed. My own argument does, I think, escape this objection. It attempts to point out a well-verified character about facts which is not simply at present unexplained in detail, but would seem to be inexplicable in terms of causation, even to a perfect knowledge. The tendency to deviate, to spread out, to produce ever new sports, is indeed in substantial agreement with the Jacobean doctrine of a growing universe. But I do not think the inadequacy of any given concepts, or group of concepts, to account for motion, change, or life, can be regarded as a proof of a real spontaneity in those facts.

And the present argument goes even further. There seems to me no ground for saying that there is anything about spontaneity which is unintelligible, *i. e.*, beyond clear conception. Chance as here defined appears to be clear enough. It is a dual affair, with a collective and an individual aspect, and in my view each of these aspects is meaningless without the other. The collection is law-abiding, the individual members, within limits, ambiguous. But I do not see why ambiguity is not a perfectly clear concept. There would seem to be, then, no real reason for excluding spontaneity from the kingdom of the intellect. It should be included as a genuine scientific category, no more wonderful than law itself. Not the limitation of the understanding by something indefinable, mysterious, unaccountable, but the inclusion of that something within the sphere of clear definition, is what every thinker naturally desires.

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EXPERIMENTAL ORAL ORTHOGENICS: AN EXPERIMENTAL INVESTIGATION OF THE EFFECTS OF DENTAL TREATMENT ON MENTAL EFFICIENCY[1]

LITTLE if any attempt has hitherto been made to measure by scientific, objective means the mental improvement resulting from the correction or removal of the various physical defects which are now generally known to afflict the majority of school children. We are beginning to appreciate, from a number of recent studies, the extent of the retarding effect upon mental growth of such physical anomalies as adenoids, hypertrophied tonsils, nasal obstructions, defective ears, eyes, and mouths; but no one has attempted to deter-

[1] Read before Section L, Education, of the American Association for the Advancement of Science, Washington, December 29, 1911.

mine experimentally the precise orthogenic effects which are believed to ensue from a definite course of combined prophylactic and operative treatment. And yet our whole system of medical school inspection and treatment must ultimately justify itself by its *demonstrated, verifiable* results—not by the opinions and assumptions, based on unaided observation, of schoolmasters, or medical inspectors, or school patrons, but by the comparable scores of a system of verifiable and demonstrable objective measures.

In the present paper we shall give a very brief sketch of the results of an attempt to determine by scientific, mental measures the influence of hygienic and operative dental treatment upon the intellectual efficiency and working capacity of a squad of 27 public-school children in Cleveland, Ohio (10 boys and 17 girls), all of whom were handicapped, to a considerable degree, with diseased dentures or gums and an insanitary oral cavity.[2] These children were the recipients of free dental treatment at the hands of the Cleveland Dental Society and the National Dental Association during the course of the experimental year, which began in May, 1910, and closed in May, 1911. The treatment included not only the filling of dental cavities, the treatment of the gums, the brushing of the teeth and gums after each meal, and the sanitation of the oral cavity, but the thorough fletcherizing of the food. Oral euthenics contemplates not only mouth sanitation and the carpentry of the teeth, but the complete mastication of the food. Instruction relating to mouth hygiene and correct eating habits was given at the school (Marion) by the chairman of the Oral Hygiene Committee of the National Dental Association (Dr. W. G. Ebersole), together with two demonstration meals. Follow-up work was done by an employed nurse, for the purpose of giving individual advice and instruction to parents and pupils, and for the purpose of ascertaining whether the pupils were faithfully following the instructions.

This research, it may be added, was the outgrowth of the nation-wide school oral-hygiene campaign inaugurated in Cleveland in March, 1910, by the National Dental Association. My own connection with the movement consisted in suggesting, contriving and giving (in person or by proxy) five series of psychological efficiency tests at stated intervals during the experimental year. These tests were designed to measure any improvement or increase, which might result from the practise of the oral hygiene regimen sketched above, in the power of immediate recall (immediate visual memory span), in the capacity to form spontaneous and controlled associations, in the

[2] A more complete discussion of this research appears in "Experimental Oral Euthenics," *The Dental Cosmos,* April and May, 1911, pages 404 ff. and pages 545 ff.

ability to add, and in the ability to perceive, attend, and react to, certain visual impressions.

In the memory test the pupils were required to memorize, during a period of 45 seconds, as many figures as possible. 10 figures, each containing 3 digits, in large print on a cardboard were displayed before the class. Exactly one minute was allowed for writing. This test is thus based on the use of non-sense materials and furnishes a measure of the immediate visual memory span.

In the spontaneous association test the pupils were provided with a sheet of paper containing a column of 30 simple, every-day words. At a given signal they were told to turn the papers right side up and write opposite each word the first word suggested by it, irrespective of whether or not the suggested word was logically connected with the supplied antecedent or key-word. The time allowed was 85 seconds. The number of words written in a test like this furnishes an index of the speed of ideating or forming spontaneous associations—or, in other words, of the speed of thinking.

To measure the speed of forming controlled associations an antonym test was employed. In this the pupils were supplied with a sheet containing a column of 25 key-words, opposite each of which they were instructed to write (during 85 seconds) only that word which has the opposite meaning: *e. g., better—worse; sunrise—sunset.* This test requires intelligent discrimination and demands a higher degree of associational efficiency than that required in the previous test.

In the test on the speed and accuracy of adding the pupils were supplied with a sheet containing 32 columns of figures, each column consisting of 10 one-place digits. They were told to add as many columns as possible within the time limits (2 minutes) without stopping to re-add any of the columns. This test gives a measure of the ability to form controlled numerical associations.

In the attention-perception test (A-test) a sheet was provided containing 26 lines of capital letters. The letters were printed entirely promiscuously instead of in proper alphabetical order. The pupils were told to start at the left end of the top line and proceed to draw a line through as many of the A's as possible within the time limits (100 seconds). They were specially cautioned not to skip any A's or to cross out any other letters. This test gives a measure of the speed and accuracy of perceptual discrimination, of the power of sustained attention, and, secondarily, of the speed and accuracy of manual reaction.

These five tests thus explore some of the fundamental mental traits or capacities. In all tests, and in all sittings, the pupils were uniformly urged to do their very best. A system of quantitative,

or combined quantitative and qualitative, scoring was worked out for each test.

In order that tests of this character may be used as measuring-rods for gauging the increased functional efficiency resulting from the given euthenic or corrective factor or factors, a number of essential conditions must be supplied.

First, each of the tests must be constructed in sets or series, so that some of the tests may be given before the treatment begins, and some during the course of the treatment, or after its close. In this investigation each test was arranged in six sets, numbered from 1 to 6. Tests 1 and 2 were given before treatment began. The average of these two pre-treatment tests, therefore, represents the pupils' initial efficiency. The last four tests were given during the course of the treatment, or after its close, so that the average of these represents the pupils' terminal efficiency. The difference between the two averages accordingly represents the gain (index of improvement) made during the course of the treatment. Or, instead of taking the average of the last four tests for the final efficiency, we may substitute the average of the last two. This plan seems preferable, because the last two tests were given from three to five months after the dental treatment had been completed for all the pupils, while tests 3 and 4 were given only one or two months after the beginning of the treatment for more than half of the pupils. Sufficient time had, therefore, not elapsed to allow the orthogenic effects to become operative, at least not in maximal degree, at the time of the third and fourth tests.

Secondly, the sets must be so constructed that all of the successive tests in the same set are uniformly difficult. That is, test number 2 must be of the same difficulty as test number 1, test 3 the same as test 2, and so on. Manifestly, if each of the successive tests diminishes in difficulty, the increased efficiency shown is spurious or largely exaggerated. Contrariwise, if each successive test increases in difficulty the actual improvement will be minimized or counteracted. Considerable pains were taken to make all the tests of a given set equi-difficult. Elsewhere evidence has been adduced to show that the tests were fairly uniform in difficulty.

Thirdly, the conditions of giving the tests must be strictly uniform in all the successive sittings. These conditions refer to the character of the explanations, the use of incentives or suggestions, the constant putting forth of maximal effort by the examinees, the withholding of assistance or fore-knowledge of the test materials, the seating of the pupils, the hour of the day used for testing, the time allowed for the tests, and the employment of uniform super-

visory conditions. A scrupulous attempt was made in this research to realize these requirements.

Fourthly, to place the results upon a strictly comparable basis, a second squad of *untreated* children should be given exactly the same tests under precisely the same conditions. These children should come from the same social strata as the treated children, should approximately be of the same ages and suffer from the same degree of physical handicap. By means of the data obtained from such an untreated squad we should be able to determine the amount of improvement which is due to such contributing factors as familiarity, habituation, practise, and natural development (merely growing older), and the share which is solely due to the application of the orthogenic factor under consideration. Unfortunately it was not possible for me to get such a squad as this organized during the experimental year.

Fifthly, and finally, the factor or factors whose orthophrenic influence is to be measured must be investigated under "controlled conditions." One must make certain that the factor is constantly operative in the treated squad, and that it is inoperative in the untreated squad. In this investigation the oral hygienic measures were subject to a fair degree of control. It was the duty of the employed nurse to see that the pupils conformed strictly to the requirements.

What, now, do the results show with respect to the influence of the dental treatment upon the working efficiency of the pupils? In attempting to answer this main question we shall also refer briefly to a number of accessory facts brought out in the investigation. One of these facts is the circumstance that while the boys manifested a higher degree of efficiency than the girls in all tests except the perception test, the indices of improvement were about the same for the two sexes, whence the boys' manifest superiority in the efficiency scores is not paralleled by a corresponding superiority in the improvement indices. Similarly the amount of improvement was about the same for the older and younger pupils, a result not entirely in accordance with expectation, for it is currently believed that the benefits derived from the correction of physical defects are greater the earlier in the child's career the defect is corrected. This is believed to be true particularly as regards nasopharyngeal obstructions. But so far as the mal-effects of dental defects are concerned there are no significant age differences. Pupils between the ages of 11 and 15 appear to profit in equal degree, irrespective of sex, from the broad application of community mouth hygiene.

On the other hand, the individual differences between the pupils in all tests are significant. The differences are quite as large as the differences frequently brought to light in other psychological and

pedagogical experiments on pupils of the same age or school grade. Some pupils show a high degree, others a low degree, of proficiency; and some pupils make marvellous gains while others gain very little, or not at all, or actually lose in efficiency. It is therefore apparent that experiments of this sort, which are based on only a few pupils, are at best only suggestive, and that valid inferences or conclusions must be based on the central tendencies or average results of a considerable number of pupils.

Not only do we find these large individual differences in the efficiency scores and improvement indices, but the fact that a pupil gains much in one test does not warrant the belief that he will gain much in all the other tests. Quite the reverse may be the case. Thus a list of the 5 pupils, who made the *smallest* improvements in each of the 5 tests, was found to contain 19 of the 27 pupils, while the list of the 5 pupils, who made the *greatest* gain in each of the 5 tests, included 13 pupils. But not a single pupil was enumerated among the 5 *poorest* in *all* the tests, nor was a single pupil enumerated among the 5 *best* in *all* the tests. On the other hand, 8 of the pupils, ranking with the 5 poorest gainers in one test or another, also ranked with the 5 best gainers in one test or another. While 2 of these showed little improvement in 2 tests, they nevertheless made large gains in 2 tests. It is thus apparent that many pupils who gain little in some tests may improve remarkably in others. But it is worthy of remark that only 1 of the 3 pupils who were enumerated among the *best* gainers in *3 or more* tests was included among the *poorest* gainers, while none of the 3 who were among the poorest in *3* tests took rank with the 5 best in any of the 5 tests, so that there is a certain amount of correlation between the indices of improvement in the various tests, justifying the conclusion that pupils who improve very slowly in *several* tests will not take place with the best ground-gainers in any of the tests. Such pupils are probably suffering from general impairment or marked retardation. But teachers must recognize that a child who gains little along one line of mental action may be developing normally, or even supernormally, along other lines. His capacity for development can not be determined from the improvement indices of one trait. Scientific pedagogy will make little progress until this fact is recognized, so that the educational activities may be adjusted to meet individual developmental idiosyncrasies.

Although there are these individual differences the character of the *central tendencies* is unmistakable: there is a *decided gain* in every test, and not only are the gains decidedly more frequent than the losses, but the largest gains are invariably emphatically larger than the largest losses. This may be seen from the following data

for each test, based on the average scores of tests 1 and 2, and the averages of tests 5 and 6:

Memory: 8 pupils lost in amounts varying from 5 to 15 per cent., while 19 gained in amounts varying from 0 per cent. to 116 per cent. The average gain for all pupils amounted to 19 per cent.

Spontaneous association: 2 pupils lost, the one 18 and the other 43 per cent., while 25 gained from 2 to 162 per cent. The average improvement amounted to 42 per cent.

Addition: 1 pupil suffered a loss of 13 per cent., 26 gained from 6 to 125 per cent., while the average improvement was 35 per cent.

Associating antonyms: all the pupils gained in amounts varying from 33 to 666 per cent., the average gain being 129 per cent.

Perception-attention: all gained in amounts varying from 19 to 101 per cent., the average improvement amounting to 60 per cent.

It is thus evident that the gains varied considerably in the different tests, and that the largest improvement occurred in the antonym, attention-perception, and spontaneous association tests. The average gain for all tests amounted to 57 per cent., truly a remarkably large gain.

How large a percentage of this significant gain is due solely to the improved physical condition of the pupils, which resulted from the treatment? This question does not admit of a categorical answer in the absence of parallel data from an untreated squad. But that a very large share is directly due to the dental treatment is indicated by the fact that most of these pupils were laggards or repeaters, pedagogically retarded from 1 to 4 years. During the experimental year, however, only 1 failed of promotion in the school work, while 6 completed 38 weeks' work in 24 weeks, and 1 boy did 2 years' work in 1. This indicates that the pupils' physical condition had been so bettered that they were able to profit by the instruction, to form habits from practise, and to improve mentally as a result of increasing maturity. We may therefore conclude that if it be granted that a part of the gains manifested in the psychological tests resulted from practise and increasing maturity, the gains are still significant as showing that these pupils were making normal progress during the experimental year, while many had failed to do so during the preceding year, as indicated by the records of pedagogical progress. It may be doubted, however, that the practise effects were very considerable, partly because of the brevity of the tests and the length of the intervals between some of them, and partly because of the counteracting effect of the growing monotony.

It is also significant that the regularity of attendance improved considerably during the experimental year, owing to the improved physical and mental condition of the pupils. During the preceding

year many were quite irregular, because of toothaches, bodily indispositions, chronic weariness, or distaste for the school work; 5 were obliged to carry truancy cards, while during the experimental year it was not necessary to issue any of these cards; and several boys, previously regarded as "incorrigible," became tractable and gentlemanly. The improved physical and mental health of many of the pupils, which was noticed by the teachers, commented on by the parents, and fully realized by the pupils, was also made manifest in a more buoyant spirit, a healthier complexion, and an improved disposition and deportment. That a large share of the gains in the psychological tests, say at least one half on a conservative estimate, can be directly ascribed to the oral hygienic regimen, is undoubted, I believe.

This experiment, then, furnishes the first demonstration by means of controlled, serial, experimental tests, extending throughout a calendar year, of the psycho-orthogenic effects of the application of the broad principles of community mouth hygiene. The conclusions which follow from the results of the research are of far-reaching importance to the state and nation.

There is probably no phase of the modern child-welfare movement which merits deeper scientific study by qualified experts than the relation of normative physical health and growth, and of normative pedagogical and psychical development, in school children, to a well-conceived plan of physical and mental orthogenesis. No phase of the problem of national conservation or racial euthenics more nearly affects the very fundamentals of human existence. Our greatest national asset is the normal, healthy child. It would seem that our child-welfare and social-betterment workers could more profitably apply themselves to the scientific determination of the physiological, psychological, and social causes of physical and mental inefficiency, and the discovery of scientific, corrective measures on a community basis, than to devote their resources to the mere gathering of statistical data. The largest contribution to the permanent betterment of the race will be made by those workers who will undertake, on an adequate scale, genuine, scientific investigations into the actual, demonstrated effects of the application of various orthogenic measures of a physical and mental character. No such investigations are anywhere being prosecuted on an effective basis, notwithstanding that no one *knows the actual, proven effects* on the child of the application of various physical and psychological orthogenic measures or various pedagogical methods and devices. Our knowledge in this field is largely pretense and illusion. In no field of modern enterprise has there been such a lame attempt made to *measure results scientifically,* as in education. Indeed, we do not as yet so much as possess any scientific measures of educational results: the very conception of

"measuring results in education" is a product of very recent industrial thinking. Is it not time that our large research foundations begin to treat more fairly the problems of human conservation and particularly those of child orthogenics? A million dollars spent in orthogenic investigations will accomplish immeasurably more for the welfare of the human race than tens of millions devoted to the cataloguing of the stars of the heavens or exploring the trackless wastes of the polar regions.

From the results of this investigation the conclusion is suggested that the desirability of establishing *dental clinics* in the public schools for free inspection and treatment should present itself to the taxpayer as a simple business, if not a humanitarian, proposition: the clinics are an economic means to an economic end, namely, the paying of proper dividends on the capital invested in the schools. According to the best estimates there are 6,000,000 retardates in the public schools of the country, or about one third of the entire school population. One sixth of all the pupils are repeaters. It costs the country $27,000,000 to educate every sixth child once, twice, or three times in the same grade. That part of this enormous waste, which is ascribable to the presence of those remediable physical defects in the children which exert a retarding influence upon the mental processes or which cause children to stay away from school, is entirely preventable. Is it worth while to attempt to save this waste? Is it worth anything to the child to enable him to attend school more regularly and thereby increase his chances of promotion? Is it worth while to the repeater to shorten his stay in the schools? Is it worth while to enable him to attain a higher level of academic efficiency? Is it worth while to remove physical obstacles which may lessen his efficiency for life? Is it worth while to the taxpayer to eliminate, so far as possible, the necessity for the extra financial burden which he must assume for instruction that should have been done satisfactorily the first time? There can be none but an affirmative answer. One of the means for accomplishing these desirable results appears to be the establishment of departments of orthogenics in the public schools. But these departments must be given a broader scope than are the present departments of medical inspection, and must be under the skilled direction of health officers who are experts in applied child or clinical psychology, corrective pedagogy, and preventive and corrective hygiene.

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DISCUSSION

LETTER FROM PROFESSOR POULTON

THERE are many points which it would be interesting to discuss in Mr. Francis B. Sumner's review of my book, "Charles Darwin and the Origin of Species."[1] I should, however, have abstained from troubling you were it not for Mr. Sumner's quotation of Professor Punnett's extraordinary misstatement of the modern Darwinian view.[2] For some time I had been intending to correct this curious blunder, and now that it has been quoted in your pages and even gives an ill-founded relief to Mr. Sumner, I feel that the time has come.

Professor Punnett is speaking of two African species of the Danaine genus, *Amauris,* respectively mimicked by two Nymphaline butterflies found in the same localities. The two Danaines are *Amauris niavius dominicanus* and *Amauris echeria;* the two Nymphalines, *Euralia wahlbergi* and *Euralia mima.* All four are figured on Plate VI., facing page 134 of "Mendelism." Mr. G. A. K. Marshall, in 1902,[3] suggested that the two *Euralias* are probably forms of the same species, but the proof was not finally obtained until 1909 when the late Mr. A. D. Millar, of Durban, bred both forms from a single female.[4] There is good reason to believe, as Professor Punnett states,[5] that the relationship between the two forms is Mendelian, and I can now further add that there is no doubt that *mima* is dominant and *wahlbergi* recessive. This conclusion is founded on the recent experiments of my friend, Mr. W. A. Lamborn, on the corresponding forms in the Lagos district, viz., *dubia* (=*mima*) and *anthedon* (=*wahlbergi*). Details of these experiments were communicated a few weeks ago to the Entomological Society of London, and will appear in the *Proceedings* for the present year. Now for Professor Punnett's statement: "On the modern Darwinian view certain individuals of *A. dominicanus* gradually diverged from the *dominicanus* type and eventually reached the *echeria* type, though why this should have happened does not appear to be clear. At the same time those specimens [of *Euralia*] which tended to vary in the direction of *A. echeria* in places where this species was more abundant

[1] This JOURNAL, Vol. IX., pages 159–161.

[2] "Mendelism," page 134. This, at least, is the reference in the third British edition, 1911, of Professor Punnett's work. The footnote on page 160 of THE JOURNAL OF PHILOSOPHY gives page 144.

[3] *Trans. Ent. Soc. London,* pages 491–2.

[4] *Proc. Ent. Soc.,* London, 1910, pages xiv–xvi; *Trans.,* page 498.

[5] "Mendelism," page 135.

than *A. dominicanus,* were encouraged by natural selection, and under its guiding hand the form *mima* eventually arose from *wahlbergi.*

"According to Mendelian views, on the other hand, *A. echeria* arose suddenly from *A. dominicanus* (or *vice versa*), and similarly *mima* arose suddenly from *wahlbergi* (p. 134). . . . On this view the genera *Amauris* and *Euralia* contain a similar set of pattern factors, and the conditions, whatever they may be, which bring about mutation in the former lead to the production of a similar mutation in the latter" (p. 135).

Although Professor Punnett ought to be competent to express "Mendelian views," I am pretty confident that he will be unable to find a single Mendelian writer who would accept his assumption about the origin of the two species of *Amauris.* But, however this may be, it is quite certain that no Darwinian, modern or ancient, and certainly no student of insect systematics, has committed himself to the belief that one of these two Danaine models has directly arisen from the other.

The late Dr. F. Moore, in his revision of the *Danainæ,*[6] placed *echeria* and *dominicanus* in separate genera. In this he was probably wrong, but they are certainly widely separated. *Amauris niavius niavius* of the west, together with the eastern sub-species, *niavius dominicanus,* occupies an isolated position in the genus *Amauris,* and it is absurd—I can use no milder word—to suggest that *echeria* arose directly from either of them. Hence, the whole of Professor Punnett's assumption of a parallelism in origin between model and mimic, which Mr. Sumner finds so comforting, falls to the ground.

May I say in conclusion that, although the relationship between the two mimetic forms of *Euralia* is undoubtedly Mendelian, I can not believe that one of them arose suddenly from the other? I believe that any one who looks at Professor Punnett's Plate VI. will hesitate to accept the view that the details of either of the two mimetic patterns—reproducing with great precision the pattern of a species belonging to a different sub-family—arose all at once from the other by mutation.

I have, furthermore, some evidence in support of the conclusion that the origin of the mimicry was gradual. Another closely related species, *Euralia dinarcha,* presents on the west coast of Africa two forms very roughly resembling the Danaine models which are so won-

[6] *Proc. Zool. Soc. London,* 1883, page 201. Dr. Moore placed *echeria* and an allied species in *Nebroda.* Aurivillius in his great "Rhopalocera Æthiopica" places *niavius,* including the eastern form *dominicanus,* second and *echeria* fifteenth in the genus *Amauris.*

derfully mimicked by the forms *anthedon* and *dubia* of the allied species. I very much hope that Mr. Lamborn will be able to breed *E. dinarcha,* and ascertain whether the Mendelian relationship exists between its two forms.[7] But whether this is so or not, there can be little doubt that these forms exhibit to us an initial stage in an evolutionary journey which has been carried very much further by *anthedon* and *dubia.*

There are other interesting facts which remain to be further investigated in the Mendelian relationship of these mimics. Mr. Lamborn informs me that the recessive form *anthedon* shows a well-marked tendency to appear seasonally; so that, during part of the year, he finds only this form on the wing. Then, later on, *dubia* suddenly appears. Such a phenomenon is extremely difficult to explain on ordinary Mendelian lines. Either we are faced by some undiscovered aspect of Mendel's law or the dominant form must have the power of lying dormant in some one or more of its stages, and then suddenly appearing. Against this latter hypothesis is the fact that in the seven large families bred by Mr. Lamborn, and now in the Oxford University Museum, there was not the slightest evidence of any difference between the two forms in this respect.

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PROFESSOR DEWEY'S "AWARENESS"

IT is a shame to be asking Professor Dewey to take up so much time in answering what are regarded as irrelevant questions. But he has been so good in the past that I am going to take the liberty of putting two more questions. I shall put them entirely in Mr. Dewey's own words, so far as I can; and I shall request Mr. Dewey to forget, so far as this is possible, that in my former queries I seem to him to have confused his position with my own. The two questions I wish to lay before him concern the passage on the basis of which my previous unfortunate questions were raised. That passage I shall requote here so that all the data pertinent to my present inquiries may be seen at a glance: "Of course on the theory I am

[7] Returning to Oxford at the end of the Easter vacation, I find a letter from Mr. Lamborn written March 29, 1912, from Oni Camp, near Lagos, telling me that he has now succeeded in obtaining eggs from both forms of *E. dinarcha,* and that the larvæ are doing well. We may hope for evidence, which will decide whether these two forms are a Mendelian pair, in a few weeks. I am very fortunate in having friends in the tropics who are so often able to supply us with just the very solutions for which we are looking with the utmost interest and eagerness.—E. B. P.

interested in expounding the so-called action of "consciousness" means simply the organic releases in the way of behavior which are the conditions of awareness, and which also modify its content."[1] In this sentence it seems to be asserted that organic releases in the way of behavior are the conditions of awareness.

There are two other passages, in the essay from which the above quotation is made, which must be cited before I can put my questions. "Awareness means *attention,* and attention means a crisis of some sort in an existent situation; a forking of the roads of some material, a tendency to go this way and that" (p. 73). "A mistake is literally a mishandling; a doubt is a temporary suspense and vacillation of reactions; an ambiguity is the tension of alternative, but incompatible mode of responsive treatment; an inquiry is a tentative and retrievable (because intra-organic) mode of activity entered upon prior to launching upon a knowledge which is public, ineluctable—without anchors to windward—*because* it has taken physical effect through overt action" (pp. 69–70). A comparison of these two statements has led me, perhaps mistakenly, to think that for Mr. Dewey doubt, ambiguity, and inquiry are all cases of awareness. But these cases of awareness, if indeed they be such, are all said to be characterized by what seem to me to be not organic releases, but organic inhibitions.

My two questions, now, are these: (1) Where in these cases of awareness, if they be such, are "the organic releases in the way of behavior which are the conditions of awareness"? (2) Even if it should prove to be the case that what I have called organic inhibitions are included by Mr. Dewey within the more generic term "organic releases," why are these "organic releases" called "the conditions of awareness" rather than the awareness itself? In other words, if awareness be literally these suspenses and tensions and intra-organic modes of activity, can these suspenses and tensions and intra-organic modes of activity be properly called also the *conditions* of awareness?

There are of course several other questions that I am keeping intra-organic and therefore retrievable—two anchors weighed from the windward, I have found, are enough at a time. But if the above two questions are answered, I hope that I may get from these answers a clew to the answers of the others.

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[1] "James Memorial Volume," page 69.

REVIEWS AND ABSTRACTS OF LITERATURE

Laughter: An Essay on the Meaning of the Comic. HENRI BERGSON. Authorized translation by CLAUDESLEY BRERETON and FRED ROTHWELL. New York: The Macmillan Company. Pp. vi + 200.

As usual, Professor Bergson is fortunate in his translators. There is a cockiness of expression in this version of "*Le Rire*" not altogether true to the suave dignity of the original, but the matter is such that the manner becomes it. Laughter, if Professor Bergson is right, is also cocky: an impertinence, he says somewhere, and it is with laughter that he here deals. His handling is in terms of the characteristic of Bergsonian philosophy. This is constituted by analytic dualisms of time and space, quality and quantity, life and matter. Time, quality, and life are real and potent, the very stuff and texture of existence: space, quantity, and matter are but negations and inversions thereof, mere appearances of the living onrush. The routine of the daily life, our social relations, our amusements, are combinations of this process with its negations—spatializations of time, intellectualizations of instinct, mechanizations of life. The exigencies of action make them so: they are the soul of use, and it is by its utilities that life maintains itself. There exists, however, a dimension in which utilities, with their concepts and generalizations, have no worth, where intellect is satanic rather than salvational, where only concrete and living individualities count, where the *élan vital* is encountered with no veils between. In this dimension lies the field of art, which, "whether it be painting or sculpture, poetry or music, has no other object than to brush aside utilitarian symbols, the conventional and socially accepted generalities, in short, everything that veils reality from us, in order to bring us face to face with reality itself." The older way of expressing this true and ancient doctrine is to say that art is intrinsic and expressive, the residual life extrinsic and utilitarian—sometimes.

But the art of comedy is excommunicate from this election. It deals not with individuals, but with types; it is external and observational, not internal and imaginative. Only averages are its care, and the inductive sciences its kin, in that in method and object its "observation is always external and the result always general" (p. 169). And this must be, since the essence of the comic is to be a mechanization of life, a petrifaction of the labile, a mechanization and petrifaction not, however, through and through, but capable of correction, and therefore subject thereto at the hands of laughter. But that laughter's function may be universal, its object, the comic, must be general and not individual. Comedy, hence, can not reveal reality.

Whether it is because of this metaphysical preconception that the analysis of objects of laughter is limited to French comedy from Molière to Labiche, or because such an analysis has led to this generalization in terms of the Bergsonian metaphysic, can not be easily said. Certainly, to find in addition that laughter must concern itself with something human, in its social relations; that it must be divorced from emotion, requiring a

"momentary anesthesia of the heart," points to the first alternative, for these are deducible from M. Bergson's interpretation of life and nature. And it is only such a deduction that would see the comic object everywhere as a "mechanization of life"—caricature, because it involves rigidity and disproportion of feature; repeated or inverted movements, because they have, when alive, a continually changing aspect; character, because it is funny when automatism is opposed to freedom, the persistent and unconscious self-admiration of vanity to the labile and scientific cautiousness of modesty.

Hence, it is not impossible that if M. Bergson had gone further afield for his cases of the comic, if, instead of confining himself to the comedy of literature and social life, he had sought out the occasions of laughter in nature and the other arts, he might have found it needful to modify his theory a little. Granted that it lightens the cases he cites, does it equally illuminate the laughter occasioned by tickling, by fear, by victory, by release from any kind of suppression or tension? In cases of this sort is not the *élan vital* really liberated from, rather than a victim of, the contingencies of mechanization? How does the "mechanization of life" explain the comic of music, of discords of pure colors that many artists find laughable? What human or social relation is actually to be seen in these things?

Then laughter itself—is it really "unemotional"? It is true that mirth is not anger nor pity nor horror nor joy, but need it be any the less an emotion on its own account? As well deny it of any other that has an identifiable individuality. That mirth is not a negative nor depressed emotion is obvious, that it is cruel and pitiless is often true, but then so are joy and anger among the exalted emotions, and fear, among the depressed ones. The "anesthesia" of the heart is common to all emotions, to say the least—that is why they are emotions. They are selfish, central, exclude alternatives. They consume their object, each according to its fashion. If laughter hurts, so does anger; if mirth is blind, so is joy. And just as these are not intrinsically corrective, neither is mirth. Arising first as an intrinsic expression of certain values in existence, it acquires a secondary character which is in no way essential or definitive of it. Its utility is an artifact, not a natural growth, and the other emotions can participate in a similar utility, for if people dislike being laughed at, they also dislike being stormed at or pitied, and seek to change the conditions which evoke these emotions.

Now are such conditions also mechanizations of life? And if they are not, may not some of those which evoke mirth also be innocent of that rigor? In nature there seem to be many such innocents. But even if there be one only, M. Bergson's subtle and fascinating book is rendered by it a "fallacy of composition" in which one object of mirth, viz., the petrifaction of the labile, is identified with all, and in which one incidental utility is converted into constitutive function. Yet not altogether, for at the end M. Bergson finds laughter also sympathetic, containing a "movement of relaxation," a relief from the strain of living, analogous to dream.

And perhaps in its fundamental and deeper nature, laughter is that and only that.

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The Philosophy of Music: A Comparative Investigation into the Principles of Musical Esthetics. HALBERT HAINS BRITAN. New York: Longmans, Green, & Co. 1911. Pp. xiv + 252.

After a somewhat laborious "Introduction," the treatise in hand comprises a "Psychological Analysis of the Elements of Music," with chapters on rhythm, melody, harmony, and musical expression, and a discussion of "The Philosophy of Music," considered with reference to the appeal and the content of music to musical criticism and to education.

The perspective of the "psychological analysis" may be indicated by a typical passage: "Rhythm . . . is an attribute of neural activity inbred in the nervous tissues through ages and cycles of development and growth before the mind was capable of true creative work such as both melody and harmony imply. Consequently the music of undeveloped tribes and of uncultivated taste is preponderatingly rhythmical. Instruments of percussion are the favorite musical instruments of men in the lowest stages of mental development" (p. 63). The combined authority of physiology and anthropology is characteristic of the day, but to the reviewer it seems too often to amount merely to the restatement of familiar facts in grotesque or pedantic terms, less a profit to learning than a trial of temper.

Professor Britan is better in his discussion of melody and harmony where neither protoplasm nor "primitive man" can be conveniently adjured. In melody he finds the gist of "musical thought," to which he proceeds to apply the rhetorical criteria of unity, strength, grace, originality, significance. While these terms serve no deeper purpose than to point to certain obvious features of musical composition sufficiently analogous to their literary counterparts to justify the terminology, yet in this there is a real service. For in the first place, it is worth while to suggest for musical description a set of analogies other than the overused (and often absurd) ones of painting and architecture; and in the second place, in a thoroughly profitable chapter on "Musical Criticism," Dr. Britan points the practical need and application of his terms. As to the quite different matter of penetrating the nature and analyzing the appeal of melody, it can hardly be maintained that we are much advanced.

A suggestion that invites consideration is that the plaintive effect of the minor mode is due to the primacy of the major in the general ordination of our musical conceptions: "So here in the minor scale, when we feel the unrest and yearning it produces, we are yearning in reality for the more natural order of the major mode" (p. 146). This, of course, is but another application of the "expectation" theory to musical interpretation—like all the rest, still leaving with us, unsolved, the foundation of such expectancy.

A general key to Professor Britan's position is his excellent saying,

"There are no patterns in art, though we are endeavoring to establish certain principles" (p. 217). And most of the principles laid down will be generally accepted. Yet his book as a whole would certainly be more effective without the odd assumption that it constitutes a "pioneer work" in a field represented by a literature of which his seventeen prefatory "references" give small measure.

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JOURNALS AND NEW BOOKS

THE AMERICAN JOURNAL OF PSYCHOLOGY. January, 1912. *The Relative Legibility of Different Faces of Printing Types* (pp. 1–34): BARBARA ELIZABETH ROETHLEIN. – An experiment to determine the ease or difficulty that various printing types present in reading. The factors that produce legibility are given. The texture of the paper is not important. The modification of certain letters is urged. *The Psychology of the New Britannica* (pp. 37–58): E. B. TITCHENER. – The author has made a careful study of the articles that deal with psychology in the new Britannica. He finds little to commend and much to condemn. It seems that this new edition of the encyclopedia has not made an adequate revision of its psychological material. *The Function of the Several Senses in Mental Life* (pp. 59–74): EDMUND C. SANFORD. – A brief survey of the development of the various senses is here given. Several mental experiences are taken up and discussed in relation to the various senses. *The Relation of Practise to Individual Differences* (pp. 75–88): FREDERIC LYMAN WELLS. – The experiments indicate that a superior performance at the beginning is not attained with a sacrifice of the possibility of future improvements. *The Influence of Caffein Alkaloid on the Quality and Amount of Sleep* (pp. 89–100): H. L. HOLLINGWORTH. – Small doses do not seem to disturb sleep. Doses larger than six grains impair sleep for most subjects. The effect is greatest when taken on an empty stomach. *Minor Studies from the Psychological Laboratory of Vassar College. Mediate Associations Studied by the Method of Inhibiting Associations: An Instance of the Effect of "Aufgabe"* (pp. 101–109): M. VALERIE ATHERTON and M. F. WASHBURN. *A Study of the Images Representing the Concept Meaning* (pp. 109–114): MARY W. CHAPIN and M. F. WASHBURN. *Recent Literature on Psychoanalysis* (pp. 115–139): DR. J. S. VAN TESLAAR. – A series of reviews of the following: (1) S. Freud, *Psychoanalytische Bemerkungen über einen autobiographisch beschriebenen Fall von Paranoia* (Dementia Paranoides). *Sonderabdruck aus dem Jahrbuch f. psycholanalytische und psychopathologische Forschungen,* III., 1911, 9–68. (2) Oskar Pfister, *Hysterie und Mystik bei Margaretha Ebner (1291–1351). Zeitschr. f. Psychoanalyse,* I., 1911, 468–485. (3) S. F. Ferenczi, *Anatole France als Analytiker. Zentralblatt f. Psychoanalyse,* I., 1911, 461–467. (4) Otto Rank, *Das Verlieren als Symptomhandlung. Zentralblatt f. Psychoanalyse,* I., 1911, 450–460. (5) Albert Mohl, *Beruhmte Homosexuelle. Grenzfragen des Nerven und Seelen-*

lebens, LXXV., 1910, pp. 80. (6) H. Bertschlinger, *Heiligungsvorgänge bei Schizophrenen. Allgem. Zeitschr. f. Psychiatrie,* LXVIII., 1911, 209–222. (7) S. Freud, *Formulierung ueber die zwei Prinzipien des psychischen Geschehens. Sonderabdruck aus dem Jahrbuch fur psychoanalytische und psychopathologische Forschungen,* III., 1911, 1–8. (8) Oskar Pfister, *Die psychologische Entratselung der religiosen Glossolalie und der automatischen Kryptographie. Sonderabdruck aus dem Jahrbuch f. psychoanalytische und psychopathologische Forschungen,* III., 1911, 427–466. (9) M. Wulff, *Beiträge zur infantilen Sexualität. Zentralblatt f. Psychoanalyse,* II., 1911, 6–7. (10) Jan Nelken, *Ueber schyzophrene Wortzerlegungen. Zentralblatt f. Psychoanalyse,* II., 1911, 1–5. *Alfred Binet* (140–141).–A brief biographical sketch. *Book Reviews.* E. L. Thorndike, *Animal Intelligence:* L. W. SACKETT. C. S. Myers, *A Text-book of Experimental Psychology with Laboratory Exercises:* E. B. T. H. H. Britan, *The Philosophy of Music:* E. B. T. H. Bergson (translated by C. Brereton and F. Rothwell), *Laughter; an Essay on the Meaning of the Comic:* E. B. T. J. Welton, *The Psychology of Education:* W. S. FOSTER. William Brown, *The Essentials of Mental Measurement:* W. S. FOSTER. H. Addington Bruce, *Scientific Mental Healing:* W. S. FOSTER. Francisco Redi (translated by M. Bigelow), *Experiments in the Generation of Insects.* H. de Vries (translated by C. S. Gager), *Intracellular Pangenesis; Including a Paper on Fertilization and Hybridization.* R. C. Punnett, *Mendelism.* F. L. Wells and A. Forbes, *On Certain Electrical Processes in the Human Body and Their Relation to Emotional Reactions.* M. T. Whitley, *An Empirical Study of Certain Tests for Individual Differences.* E. Abramowski, *L'Analyse physiologique de la perception.* F. Boas, *Handbook of American Indian Languages.* J. R. Swanton, *Indian Tribes of the Lower Mississippi Valley and Adjacent Coast of the Gulf of Mexico.* C. Thomas, *Indian Languages of Mexico and Central America and their Geographical Distribution.* J. W. Fewkes, *Preliminary Report on a Visit to the Navaho National Monument, Arizona.* J. W. Fewkes, *Antiquities of the Mesa Verde National Park: Cliff Palace.* W. Goodsell, *The Conflict of Naturalism with Humanism.* W. L. Rabenort, *Spinoza as Educator.* T. Schroeder, *"Obscene" Literature and Constitutional Law: a Forensic Defense of Freedom of the Press. The Social Evil in Chicago: a Study of Existing Conditions by the Vice Commission of Chicago. Report of the Vice Commission of Minneapolis to His Honor J. C. Haynes, Mayor.* W. J. Chidley, *The Answer.* G. E. Partridge, *An Outline of Individual Study.* W. Benett, *Justice and Happiness.* J. Rehmke, *Zur Lehre vom Gemüt.* J. W. H. Allen, *The Universities of Ancient Greece.* M. Offner, *Die geistige Ermüdung.* M. Offner (translated by G. M. Whipple), *Mental Fatigue.* M. Offner, *Dass Gedächtniss.* M. E. Thompson, *Psychology and Pedagogy of Writing.* W. H. Winch, *When Should a Child Begin School?* J. E. W. Wallin, *Spelling Efficiency in Relation to Age, Grade and Sex.* H. E. Cushman, *A Beginner's History of Philosophy.* L. J. Walker, *Theories of Knowledge: Absolutism, Pragmatism, Realism.* C. J. Deter, *Abriss der Geschichte der Philosophie.* F. Cumont, *The Oriental Relig-*

ions in Roman Paganism. L. Busse, *Die Weltanschauungen der grossen Philosophen der Neuzeit.* P. Smith, *The Life and Letters of Martin Luther. Book Notes.* H. v. Buttel-Reepen, *Aus dem Werdegang der Menschheit.* Gina Lombroso-Ferrero, *Criminal Man According to the Classification of Cesare Lombroso.* Otto Klemm, *Geschichte der Psychologie.* William E. Castle, *Heredity in Relation to Evolution and Animal Breeding.* M. Sopote, *The Grades of Life.* Arthur F. Hertz, *The Goulstonian Lectures on the Sensibility of the Alimentary Canal.* James Allen, *Man, King of Mind, Body, and Circumstance.* Richard Hamann, *Asthetik.* George Trumbull Ladd and Robert Sessions Woodworth, *Elements of Physiological Psychology.* William McDougall, *Body and Mind.* George Drayton Strayer, *A Brief Course in the Teaching Process.* Edward L. Thorndike, *Animal Intelligence.* George Trumbull Ladd, *The Teacher's Practical Philosophy.* H. H. Schroeder, *The Psychology of Conduct.* M. Mignard, *La Joie Passive.* H. Addington Bruce, *Scientific Mental Healing.* Gustave F. Mertins, *A Watcher of the Skies.*

Biuso, C. Prolegomeni ad una Psicodinamica. Rome: Albrighi, Segati, & C. 1912. Pp. 176. 2.50 L.

Bosanquet, B. The Principle of Individuality and Value. The Gifford Lectures for 1911. London: The Macmillan Company. Pp. xxxvii + 409. 10s.

NOTES AND NEWS

M. HENRI POINCARÉ'S lecture at the Sorbonne on April 12 was as brilliant as it was instructive. He dealt mainly with the constitution of matter, and drew the attention of his hearers, the French Physical Society, to the objective reality of the chemical atom, which he considers to be now beyond dispute. He made a bold comparison of the free electrons within the atom to comets, while considering the tied electrons as equivalent to the fixed stars, and accepted the magneton of M. Weiss as the third component of matter. Hence, he said, we must consider the atom, if we accept the most probable hypotheses current, not as a system whose movements are ordered and ruled by definite laws, but as a world where reigns a disordered agitation of elements delivered over to chance. Yet this world is rigorously closed to us at present, and every atom constitutes, according to him, an "individual." M. Poincaré's lecture will do much to clarify the views of inquirers into the subject, and it is to be hoped that during his forthcoming visit to this country he may repeat some of the conclusions announced in it.—*Athenæum,* April 27.

PROFESSOR WILLIAM JAMES'S letters are being collected for biographical purposes, and any one who has any of his letters can render assistance that will be highly appreciated by addressing Henry James, Jr., 95 Irving St., Cambridge, Mass. Casual or brief letters may have an interest or importance not apparent to the person preserving them; and news of the whereabouts of any of the late William James's letters will be gratefully received.